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Discussions

Interactive comment on "Evaluation of Release-05 GRACE time-variable gravity coefficients over the Ocean" *by* D. P. Chambers and J. A. Bonin

Anonymous Referee #2

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The paper by D. Chambers and J. Bonin evaluates the accuracy of recently released time-variable GRACE gravity field coefficients over the oceans by means of a number of well-established indicators. The presentation contributes valuable feedback to both the SDS processing centers and the users of the GRACE data in a very timely manner and certainly merits publication in Ocean Sciences. I recommend the acceptance of this paper after two major issues discussed below are properly adressed.

(1) In addition to the de-striping method originally devised by Swenson and Wahr (2006), a number of alternative filtering approaches have been published during the last years that all attempt to reduce the systematic meridional stripes in the GRACE fields. For example, Kusche (2007) devised a Tychonow regularization-like method that can be applied to the GRACE Level-2 gravity fields during post-processing. The

method directly relies on the error covariance information provided by the processing centers and therefore avoids any ad-hoc tuning efforts as discussed in your section 4. Since the method by Kusche has been made available to a number of groups worldwide and has shown to work favourably for both ocean and continental applications (i.e., no signal reduction in higher latitudes), I suggest to consider it also for the analyses presented here.

(2) 96-20: The errors of JPL_ECCO and OMCT are certainly not uncorrelated, since both models share a number of common bits of information (e.g., primitive equations including Boussinesq and hydrostatic approximations are used, spatial resolution is comparable, atmospheric forcing is essentially based on identical meteorological observations that were assimilated into NWP models). Those correlations are primarily responsible for your apparently negative errors that finally led to the rather crooked blending of sigma_E-A into the final error maps. I suggest eighter to remove this error assessment section completely, or at least replace the apparent negative errors by NaN values in the plots in order to make clear where this error assessment method obviously fails. In any case, the assumption of uncorrelated errors needs to be emphazised and critically discussed.

A number of minor comments might be considered during the revision of the paper:

90-18: C_20 coefficients of RL05 were found to be much more consistent with SLR then before, a replacement of those coefficients does not appear to be necessary anymore. Your suggestion on this issue might be valuable for the paper.

91-5: From my understanding, the method has been developed by Swenson and Wahr (2006), whereas Chambers (2006) only suggested to slightly modify the way the filtering coefficients are obtained. Calling this an 'algorithm development' is certainly an overstatement.

93-18: There are substantial differences among OMCT RL04 and RL05, but those where not implemented to "correct obvious deficiencies", which where not so obvious

only some time ago. Instead, since OMCT RL04 is based on a model version that were not changed since 2006, RL05 incorporates improvements from both an increased spatial resolution as well as various changes in parametrizations obtained from validating the model against a number of data-sets not available in current quantities before 2006. RL05 can be therefore seen as an evolution of RL04, not simply a version that has been bugfixed as it might be anticipated by your formulation.

95-12: It might be worth to investigate reasons for this discrepancy in the Arctic. Are there differences in the tide models applied by the different centers?

97-3: The errors are certainly not "well-behaved" in a mathematical sense. Please re-formulate.

97-10: The comparisons shown here might only be used as an indicator of the reliability of monthly mean bottom pressure fields. It should be made clear that overestimating the monthly mean in OMCT RL05 (with respect to an assimilated ocean model) does not necessarily imply a poor prediction of sub-monthly variability, which is effectively important for sucessful de-aliasing. This might be assessed wrt. daily satellite altimetry maps as done in your previous work, but it is certainly not the scope of this study.

Interactive comment on Ocean Sci. Discuss., 9, 2187, 2012.